ON TIDAL PREDICTION.[1]

THE moon and the earth revolve in an orbit about their centre of inertia, and the centrifugal force due to the revolution exercises an unequal repulsion on the various parts of the earth; the moon also attracts the nearer parts of the earth more strongly than the remote parts. The differential action arising from these two forces is the cause of the tides.

This is explained in many elementary books, and although the exposition is often obscure rather than lucid, I will not stop to go over the demonstration, but will merely refer to Fig. 1, which shows the direction and relative intensity of the tide-generating force at various parts of the surface of a planet.

It is obvious, from the figure, that if the system were at rest the ocean would be elongated towards and away from the satellite, and equally depressed all round the sides. This result is expressed in technical language by saying that the figure of equilibrium is a spheroid with its axis of symmetry pointed at the satellite.

Newton, the founder of the equilibrium theory of the tides, was well aware that this theory would give no approximation to the truth when the system is endowed with motion, and when the ocean is interrupted by land. But whilst he recognized that the theory was useless as directly furnishing a method of tidal prediction, he was clearly aware that it was convenient to retain the conception of the figure which an ocean would assume if the whole system were at rest. In this view he was right, and all that can be attained in tidal science rests on the equilibrium theory. It is true that the word theory is not a good one to express what is meant; but if it had been called "the equilibrium specification of tidal forces," the objections which have been raised against it would have been obviated.

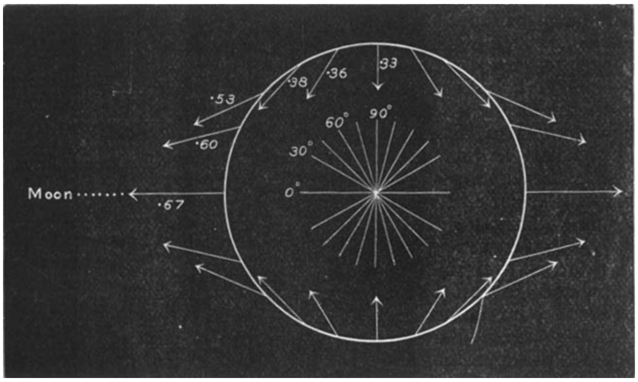

Fig. 1.—Diagram of tide-generating forces.]

The meaning of this is that, if the ocean covered the whole earth, and if all the motions were very slow, the tides would be such as are determined by the equilibrium theory. Since the intensity and direction of tide-generating forces are independent of the velocities of the earth's rotation and of the moon's motion, and also of the shape and size of the oceans, the so-called equilibrium tide serves to specify the tidal forces in the actual case. It is accordingly convenient to speak of the equilibrium tide as of a real phenomenon, and to make the transition to the actual oscillations of the sea at a later stage of the discussion.

We have only as yet spoken of the lunar equilibrium tidal spheroid, but for similar reasons there must also be a solar spheroid, of a smaller degree of elongation or ellipticity. The superposition of these two spheroids specifies the total resultant tidal force.

Since the major axes of the lunar and solar spheroids point to their respective luminaries, and since the earth rotates, the motion of the resultant spheroid relatively to the earth's surface, and the law of variation of its ellipticity, are very complex; further analysis of the two spheroids becomes, therefore, necessary. It is, accordingly, usual to regard the lunar spheroid as being built up out of three others, as follows: first, a prolate spheroid with its major axis in the equator in the same meridian as the moon; secondly, a prolate spheroid with its major axis in latitude 45°, also in the same meridian as the moon, the latitude being north or south, according as the moon is north or south of the equator; thirdly, an oblate spheroid with its axis coincident with the polar axis of the earth.

The axes of the first and second of these spheroids always follow the moon's meridian, and the ellipticities of all three vary slowly as the moon changes her linear distance from the earth and her angular distance from the equator. The ellipticity of the first or equatorial spheroid oscillates about a mean value; but the ellipticity of the second kind of spheroid is greatest when the moon is furthest north, diminishes to nothing (so that the spheroid becomes a sphere) when the moon is on the equator, and its axis leaps from 45° N. latitude to 45° S. latitude at the moment that the moon crosses from north to south, when the ellipticity begins to increase again.

The third kind of spheroid is of comparatively little importance in tidal theory, and I shall not make further reference to it.

Each of the two lunar spheroids moves slowly in space as it follows the moon's meridian; but to an observer, who is carried rapidly round by the earth's diurnal motion, the oscillations of the ocean due to the two spheroids would have different characters. In the case of the first sort of spheroid, he would pass through *both* protuberances in the course of one rotation of the earth, whilst he would only meet one protuberance of the second sort of spheroid, on account of its unsymmetrical position with regard to the earth's axis. Thus the first sort would by itself give him two high waters *per diem*, but the second sort would only give him one high water. The first may therefore be called the lunar semi-diurnal spheroid, and the second the lunar diurnal spheroid.

A similar analysis of the total solar spheroid gives a solar semi-diurnal and a solar diurnal spheroid; and the third sort of solar spheroid may again be omitted from consideration.

The axes of the two lunar and of the two solar spheroids follow the moon's and sun's meridians respectively, and their ellipticities vary with the linear distances of the two bodies from the earth, and with their angular distances from the equator. Taking these spheroids as specifications of tidal force, we see that the lunar and solar tidal influences are not yet analyzed into forces which are regularly periodic, either in intensity or in period. Accordingly, the next step is to break up each of the four variable spheroids into a number of new ones, which have invariable ellipticity and which move with uniform velocity relatively to the earth's surface. It is theoretically necessary to take an infinite number of these new spheroids to build up each of the former ones, but about five new spheroids suffice to represent any one of the old ones so closely that the error is negligible.

The result of this analysis of tidal forces is, then, to get some twenty equilibrium spheroids, which do not any longer follow the moon's or sun's meridian, but which do move with uniform velocity over the earth, and which are invariable in shape. These spheroids are, like the former ones, divisible into two groups—a semi-diurnal group, each of which would, by itself, make high water twice in a period not much different from a day, and a diurnal group, giving high water about once a day.

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[1] A paper or lecture delivered to the Cambridge Philosophical Society on February 23, 1891. A few passages in it have already appeared in Nature, in the account of the Bakerian Lecture delivered to the Royal Society on January 29, 1891. (See Phil. Trans. Roy. Soc., 1891.)

These tidal spheroids are merely specifications of the tidal forces, and may be used in the actual case with the same confidence as if the sea covered the whole earth, and as if the system were at rest. The spheroids, in fact, indicate that if the globe were covered with water, and rotated very slowly, there would be regularly periodic rises and falls of water equal to those given by each of the ideal equilibrium spheroids. But the determination of the oscillations of a sea of variable depth and irregular outline on a rotating planet quite surpasses the power of mathematical analysis, even when the generating forces are regularly periodic. It can only be asserted that at each place there will actually occur a regular rise and fall of the sea with unknown amplitude, exactly co-periodic with the passage of the equilibrium spheroid, and that high water will follow the crest of the ideal equilibrium spheroid by some unknown but constant interval. It may, however, be certainly concluded that if two spheroids of the same class—both semi-diurnal, or both diurnal—move with nearly equal speed over the earth, then the heights of the corresponding high waters will be nearly proportional to the ellipticities of the respective spheroids, and the intervals of retardation will be nearly equal to one another. It follows from this law that an equilibrium spheroid, which varies slowly in ellipticity, and which moves nearly uniformly over the earth, will correspond at any place with a real oscillation of the sea, of amplitude nearly proportional to the varying ellipticity, and with nearly constant retardation of high water behind the passage of the crest of the equilibrium tide. The more rapidly the ellipticity varies, and the more irregular the motion of the spheroid, the less accurate does this law become.

Now this brings us to consider two methods of treating tidal observation and prediction—namely, the synthetic and the analytic methods.

In the synthetic method all the semi-diurnal spheroids are added together to form a resultant luni-solar semi-diurnal spheroid, and a similar recomposition of the other group gives a resultant luni-solar diurnal spheroid. These two resultant spheroids are found to move over the earth with some approach to uniformity, and their ellipticities vary with moderate slowness. It is, then, assumed that at any port the heights of the semi-diurnal and diurnal tide-waves will vary *pari passu* with the ellipticities of the two spheroids, and that the retardations in time will be nearly constant.

In the analytic method as many tidal spheroids are considered as are wanted to give a sufficiently accurate representation of the tidal forces. Each one of these constituent spheroids is then known to give rise to a mode of oscillation of the sea, which at any port is constant in amplitude and in retardation.

In both plans astronomical considerations are essential: in the synthetic method the laws of the variation of the ellipticity and of the speed of motion are dependent on the positions of the moon and sun; and in the analytic method we have to determine how many different spheroids of constant ellipticity are wanted to build up the resultant tidal spheroid with sufficient accuracy, and what are their various uniform velocities over the earth's surface.

In the synthetic method there is a single semi-diurnal and a single diurnal spheroid, and the singleness of the spheroids is the important consideration.

In the analytic method the number of spheroids is immaterial, whilst the constancy of their ellipticities and the uniformity of their motions are the objects aimed at.

On account of this difference of aim, it is necessary to fix the positions of the moon and sun in different ways in the two methods. In the synthetic method the positions are determined by angles which bear a simple relationship to the earth's surface; in the analytic method by angles which increase at a uniform rate.

A point on the earth is fixed by its longitude and latitude, but when this system is applied to a celestial object, its longitude, measured from the meridian of the place under consideration, is called the local hour angle, and its latitude is called the declination. Accordingly, in the synthetic method the semi-diurnal and diurnal spheroids have their ellipticities and velocities specified by means of the hour angles, declinations, and linear distances (or horizontal parallaxes) of the moon and sun. There are apparently six variables, but as both the sun's declination and its parallax depend on the time of year, they are equivalent to only five variables.

The principal variations of the total semi-diurnal spheroid depend on the two hour angles, but there are also subordinate changes due to variations in the two declinations and the two parallaxes.

In the case of the diurnal spheroid we must regard not only the two hour angles but also the moon's declination as principal variables, whilst the changes due to the sun's declination and to the two parallaxes remain subordinate.

It happens that on the eastern coasts of the North Atlantic the diurnal spheroid gives rise to very little oscillation of sea-level, and therefore in Europe the two hour angles may be regarded as principal variables, and the two declinations and parallaxes as subordinate ones. The apparent time of the moon's transit being only another name for the excess of the sun's hour angle over the moon's, it is easy to regard the time of moon's transit and the interval thereafter as the two principal variables, instead of the hour angles themselves. The passage of the luni-solar equilibrium semi-diurnal spheroid over the meridian of any place bears an intimate relationship to the time of moon's transit at that place; accordingly, in the synthetic method, the interval after moon's transit and the height of rise are given as specifying the tide. The interval and height are afterwards corrected according to the declinations and parallaxes of the two bodies.

At places where the diurnal tide is small, the synthetic method possesses a great advantage—namely, that the inequalities in the height and interval are so regular in each half lunation, that a single table arranged according to the hour of moon's transit, is sufficient; there are, of course, also tables of corrections arranged according to the declinations and parallaxes of the moon and sun.

We have seen, however, that as concerns the diurnal spheroid, the moon's declination should rank as a principal variable, and the application of the synthetic method to non-European ports has been an attempt to keep the moon's declination down to its supposed proper position as a subordinate variable. In this attempt the corrections have shown an ever-increasing tendency to complication, and there have been constructed correctional tables depending not only on the two declinations and two parallaxes, but also on the rates of change of the moon's right ascension, declination, and parallax. Even with all this care the results are not easily made satisfactory for places with a large diurnal tide. As late as 1840, Whewell had not realized that European tides are abnormal in their simplicity. In a later memoir he was evidently surprised at the importance of the diurnal tide in other parts of the world, but he seems always to have regarded it as a matter to be treated by means of corrections.

In order to prove how futile corrections must be in such cases, I refer to Fig. 2, which exhibits the rise and fall of water during one day at Aden when the moon crosses the meridian at 6 p.m. in the middle of March. In the synthetic method we should have to suppose as a first approximation that there were two equal high waters and two equal low waters in the day. In this tide curve traces of the second high water and of the second low water are seen in the lingering of the water a little above mean water-level during about four hours—namely, from 12½h. to 16½h.

The same thing is shown in even a more striking way by Fig. 3, which gives the fortnightly inequality in the height and interval during any half lunation at Portsmouth, and also shows the same inequalities during two different whole lunations at Aden.

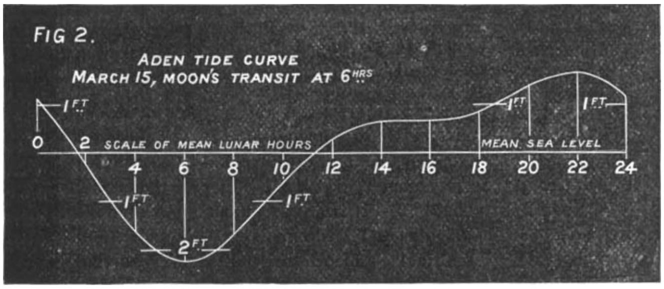
FIG 2.

In these figures the horizontal line is a scale of the times of the moon's meridian passage. When the transit is at 0h. or noon, it is change of moon; when at 12h. or midnight, it is full moon; and transits at 6h. and 18h. are the half moons, waxing and waning. In the curves of intervals the ordinates are the number of hours after moon's transit to high water; and in the curves of heights the ordinates give the number of feet of rise at high water.

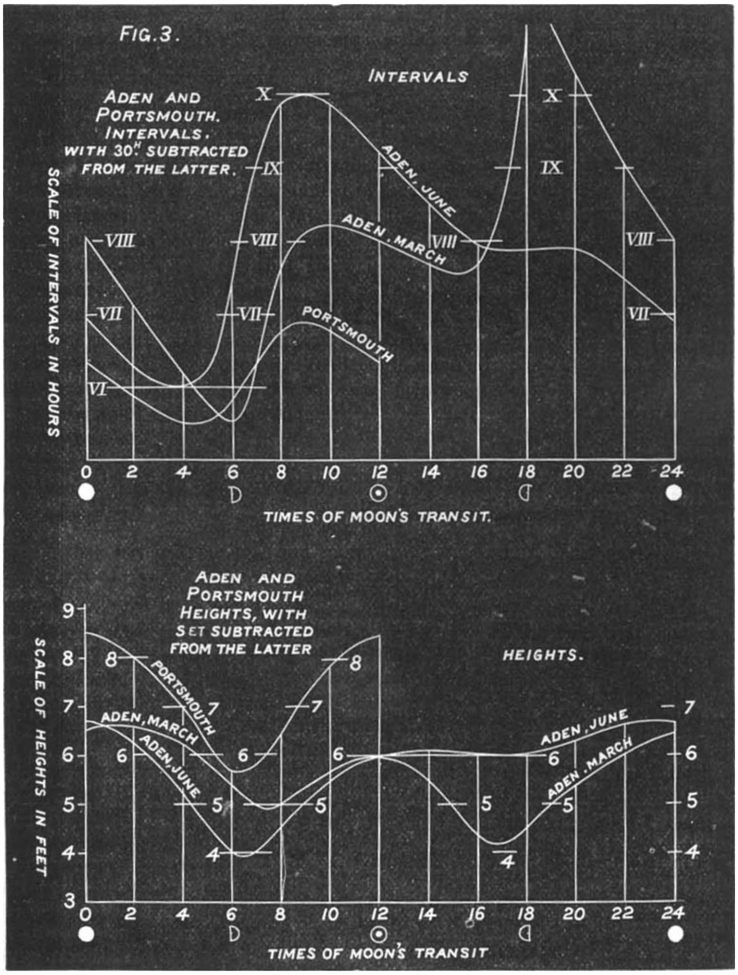
FIG. 3.

The Aden curves only show the height and interval for the high water which follows the moon's upper or visible transit, and the same curves would have to be repeated with their second halves in the first place, and with their first halves in the last place, in order to give the height and interval for the high water following the moon's lower or invisible transit.

If the synthetic method were applied to Aden, we must as a first approximation regard the March curve and the June curve as identical, and the second halves of each as a repetition of the first halves. It is obvious that it is impossible to work with any pretence to accuracy on such an hypothesis.

It will be noticed that the March curve of intervals is interrupted about the time when the moon's transit is at 18h.; this means that the corresponding high water is missing. Fig. 2, in fact, has exhibited that one of the two high waters is missing. The missing or evanescent high water is the one which ought to have followed moon's transit at 18h., and the existent one follows moon's transit at 6h.[1] Great as is the difference between the two halves of the March curve of intervals, it is less conspicuous than the contrast between the two halves of the June curve.

Finally, whilst the greatest interval exceeds the least by about an hour and a half at Portsmouth, it exceeds by nearly six hours at Aden.

The contrast between the March and June curves of heights for Aden is not so striking as in the case of the intervals, but is still very great.

Enough has now been said to show the necessity for a new departure in the really normal case of a considerable diurnal tide, and the impulse was given by Sir William Thomson when he proposed the harmonic analysis of tidal observations. This is, in fact, the analytic method the principles of which have been already sketched.

In the harmonic method, about twenty equilibrium spheroids are taken as giving a sufficiently accurate representation of the tidal forces. It has already been shown that constancy of ellipticity and uniformity of velocity are the leading considerations, and that the speeds of the spheroids are to be expressed by means of angles or quantities which increase at a uniform rate. Accordingly, the speeds are now expressed in terms of the mean solar time, and of the mean longitudes of the moon, of the sun, of the lunar perigee, of the lunar node, and of the solar perigee—all of which increase at a uniform rate. Since the longitude of the solar perigee is to all intents a constant, there are five variables instead of the six of the synthetic method.

Only three of these variables—namely, the time and the mean longitudes of the moon and sun—are required to express the speeds of the spheroids of the largest ellipticity; whilst the speeds of the less important spheroids require also the longitudes of the moon's perigee and node.[1] There are, therefore, three principal variables and two subordinate ones, but no advantage is taken of this subordination, each spheroid being treated by itself.

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[1] The order of these two high waters is apparently inverted in the figure, because the first ordinate corresponds to a time 7h. 54m. after moon's transit, being the moment of "mean lunar semi-diurnal high water." The high water which follows the transit at 6h., has just passed when the figure begins.

Each spheroid, in its passage over the earth corresponds with regularly alternating tidal forces, which generate regular oscillations of the sea of unknown amplitudes and retardations. Observation of the tides at any port affords the means of determining the amplitude and retardation of each mode of motion, and, accordingly, there are two tidal constants corresponding to each equilibrium spheroid.

The separation of tidal oscillations into some fifteen or twenty distinct simple oscillations is admirable as a method of analysis and registry, but its advantages for the purpose of prediction are less conspicuous. Since high and low water are single events arising from a number of separate causes, synthesis is essential to prediction, and the computation of a tide-table involves the determination of nearly four maxima and minima per diem of an algebraic expression of fifteen or twenty terms. Dr. Börgen, of Wilhelmshaven, has attacked the problem with courage and success, but the work remains, and always must remain, very laborious. In fact, prediction in the harmonic system labours under one great disadvantage—it is very expensive.

Sir William Thomson was so conscious of this disadvantage, that, shortly after his initiation of the harmonic method, he suggested that the obstacle might be turned by the mechanical synthesis of the separate oscillations. It was in 1872, I believe, that he laid down the principles upon which a synthetic machine might be constructed. Mr. Edward Roberts, of the *Nautical Almanac* Office, bore a very important part in the realization of the idea, and the tide-predicting instrument, now in the Indian Store Department at Lambeth, was constructed by Légé, under his direction. A paper by Sir William Thomson, in the sixty-fifth volume of the Transactions of the Institution of Civil Engineers, and the subsequent discussion, contain details of the respective parts played by Sir William Thomson, Mr. Roberts, and Messrs. Légé in the realization of the idea.

The machine cost several thousands of pounds,[2] and it is the only one of its kind. It requires skill and care in manipulation, and it has been ably worked by Mr. Roberts for the production of tide-tables for the Indian Government, ever since its completion. Tables for thirty-one ports in the Indian Ocean are now being mechanically computed and published annually.

A whole lecture would be required for a full description of the instrument, and I will confine myself to a diagrammatic illustration of the principles involved.

A cord passes over and under a succession of pulleys, being fixed at one end and having at the other end a pen which touches a revolving drum. If all the pulleys but one be fixed, and if that one executes a simple harmonic motion up and down, the pen will execute the same motion with half amplitude. If a second pulley be now given an harmonic motion, the pen takes it up also with half amplitude. The same is true if all the pulleys are in harmonic motion. Thus the pen sums them all up, and leaves a trace on the revolving drum. When the drum and pulleys are so geared that the angular motion of the drum is proportional to mean solar time whilst the harmonic motions of the pulleys correspond in range and phase to all the important lunar and solar tides, the trace on the drum is a tide curve, from which a tide-table may be constructed. The harmonic motion of the pulley is given by an arrangement indicated only in the case of the lower pulley in Fig. 4. The pulley frame has attached to its vertical portion a horizontal slot, in which slides a pin fixed to a wheel. If, whilst the drum turns through one mean solar hour, the wheel turns through two mean lunar hours, the pulley executes lunar semi-diurnal oscillations. When the throw of the pin and its angular position are adjusted so as to correspond with the range and phase of the observed lunar semi-diurnal tide, the oscillation of the pulley remains rigorously accurate for that tide for all future time, if the gearing be rigorously accurate, and with all needful accuracy for some ten years of tide with gearing as practically constructed. The upper pulleys have to be carefully counterpoised, as indicated. It has not been found that any appreciable disturbance is caused by the inertia of the moving parts, even when the speed of working is high. It requires about four hours to run off a year's tides. The Indian instrument combines twenty different harmonic motions.

But, notwithstanding its admirable construction, the tide-predicting instrument mitigates rather than annihilates the disadvantages of the harmonic method. For, besides the large initial capital expenditure, the expenses of running off the curve, of the measurement of maxima and minima, of verification and of printing, are so considerable that the instrument cannot, or at least will not, be used for minor ports in remote places. It is, besides, not impossible that national pride may deter the naval authorities of other nations from sending to London for their predictions.

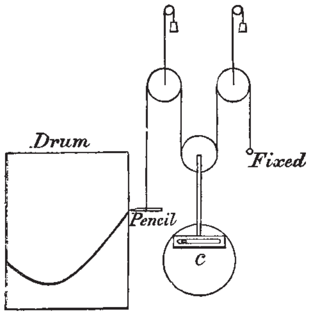

FIG. 4.—Diagram of tide-predicting machine.

There is, then, a want of other methods of forming tide-tables, if they can be devised. Now, a tide-table for any port is not necessarily of the kind produced by the machine, for such tables may be divided into two classes, which may be called the general and the special. In a special table the times and heights of high and low water are specially predicted for each day of each future year. This is the kind of table furnished by the machine, but although it is the table which a sailor likes best, it is too expensive for universal use. A general tide-table, on the other hand, merely states the law of the tides in such a form that, by reference to the *Nautical Almanac* and a little simple arithmetic, a few tides may be quickly computed for the days required.

Such tables may be computed once for all, so that the expense of producing them is of little importance; they may be printed and published along with sailing directions, and they serve for all future time.

General tide-tables have for years past been given for places where there is no diurnal tide, and where the synthetic method is easily applicable. In such a case the sailor finds the apparent time of moon's transit, and looks out in the tide-table the height and interval corresponding to that time. He then adds the interval to the mean time of moon's transit, and he has thus obtained the approximate time and height of high water. Tables of corrections according to the moon's declination, moon's parallax, and the time of year furnish him with the means of obtaining the correct time and height.

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[1] In actual use the method is partially synthetic, because the ellipticities and speeds of all the spheroids of lunar origin are taken as varying to a very small extent with the longitude of the moon's node, which completes its circuit in about nineteen years.

[2] Sir W. Thomson tells me that with a different sort of gearing the expense of construction may be much diminished.

I do not think, however, that any general tide-tables, worthy of the name, have hitherto been made for ports with a large diurnal tide, and these are the majority of all ports. It is the object, then, of this paper to show how such tables may be formed. The tables are, unfortunately, not nearly so short as when the tides are simply semi-diurnal, and the expense of computing them will be not inconsiderable, but it will be an expense incurred once for all for each port.[1] The system proposed will not render the tide-predicting instrument less useful, but it will fulfil other requirements, and will, I think, obviate the disadvantage which seemed inherent to the analytic or harmonic method.

A sketch of the manner in which it is proposed to compute these tables, and of their form when completed, will now be given.

In the complete harmonic analysis of the tides there are three principal variables—namely, the mean solar time and the mean longitudes of the moon and sun; and two subordinate ones—namely, the mean longitudes of the lunar perigee and node. In order to effect a partial synthesis of the harmonic spheroids, I introduce the conception of a fictitious satellite which moves along the equator in exactly the same way as the moon moves along the ecliptic, and which coincides with the moon at the moment that she is in the equator.[2] The time is counted from the moment at which the fictitious satellite crosses the meridian of the place of observation, so that, for each high water, time is counted from a different epoch; thus, mean solar time is replaced by the interval since fictitious transit: this is the first of the principal variables. The second variable is the moon's phase counted in degrees from 0° to 360°, or, what amounts to the same thing, the excess of the moon's true longitude above the sun's. The third variable is the time of year, or, speaking exactly, the sun's true longitude. For the subordinate variables, the longitude of the moon's node is retained as one, whilst the excess of the moon's parallax above or its defect below its mean value is taken as the other.

By means of this change of variables, the fifteen or twenty true harmonic spheroids may be replaced by three semi-diurnal and three diurnal spheroids, whose ellipticities vary within very narrow limits, and whose speeds over the earth's surface are nearly uniform. Four of these six spheroids are subject to small corrections for the variability of the moon's parallax and for the longitude of the moon's node; and the two remaining spheroids should, in strictness, be subject to corrections for the sun's parallax, which are, however, so small that they may perhaps be neglected.

The transition from the equilibrium spheroids to the actual oscillations of the sea is made by means of the harmonic tidal constants, which are supposed to have been found by the reduction of tidal observations.

Then, for any given time of year I compute the interval from fictitious transit to high water, and the height, for all phases of the moon; certain factors are also computed, by means of which the nodal and parallactic corrections to the time and height may be found.

If these computations were made at frequent intervals of the year and of the moon's phase, we should have the desired general tide-table, but it would not be given in a very convenient form, because the time of fictitious transit is not given explicitly in the *Nautical Almanac.*

The next step is, therefore, to find formulæ for the interval of time between a fictitious transit and a moon's real transit, and for the moon's phase in terms of the time of moon's transit. By means of these formulæ the tables are transformed, so that we get a table of intervals after moon's transit arranged according to the hour of transit. The table resembles that in use in the synthetic method, save that we have not to consider the moon's or sun's declinations, and that there are only two correctional tables; it is necessary, however, to compute for all times of year.

I had hoped that short tables computed at intervals of a month would have sufficed for practical purposes, but when the diurnal tide is large, the changes in the height and interval are so abrupt and so large that a skeleton table would give very inaccurate results, unless used with elaborate interpolation. We can clearly not expect sailors to use tables of double entry, in which interpolation to second and even third differences is required, and indeed all interpolation is objectionable. I therefore propose that the tables shall be made so full as to obviate interpolation; tables for about every ten days, and for every 20m. of moon's transit, seem to satisfy the requirements of the case.

The computation would run to a great length if the calculation had to be made in full, but it is fortunately possible to reduce the work to a great extent.

It appears that the height, interval, and corrections for any tide occurring after a moon's transit at any hour and at any time of year, are nearly identical with the same quantities for the tide occurring after a moon's transit twelve hours later and six months later. Thus the tables need only be formed for the half instead of for the whole year. Further, it is found to give an adequate insight into the law of tide, if the computations are actually made for each month and for each hour of transit. Since there are six months and twenty-four hours of transit, the computations are made for 144 heights, intervals, and correctional factors. There are other artifices by which the work is abridged, but into which I will not enter.

The interpolations necessary for the completion of the tables for every 20m. of moon's transit, and for every ten days, may be made with sufficient accuracy by means of curves. Although this work is tedious, it is better to throw it on the professional computer, who does it once for all, rather than on the sailor, who would do it piecemeal and many times over on various occasions. It has occurred to me that the most succinct form in which the final results could be published would be by means of these curves without tabulation, but ten or twenty pages of a book convey the information in a much more portable form than an atlas of curves, so that I am doubtful of the advisability of the graphical form of tabulation. I may here remark that although the general tide-table is primarily intended for use in the general form, it may also be used for the computation of a special tide-table without heavy expense.

Such a piece of work as this can only be deemed complete when an example has been worked out to test the accuracy of the tidal prediction, and when rules have been drawn up for the arithmetical processes, forming a complete code of instructions to the computer.

I chose the port of Aden for the example, because its tides are more complex and apparently irregular than those of any other place which, as far as I know, has been thoroughly treated. The tidal constants for Aden are well determined, and the annual tide-tables of the Indian Government afford the means of comparison between my predictions and those of the tide-predicting instrument. The arithmetic of the example was long, and the plan of marshalling the work was rearranged many times. An ordinary computer is said to work best when he is ignorant of the meaning of his work, but in this kind of tentative work a satisfactory arrangement cannot be attained without a full comprehension of the reason of the methods. I was, therefore, very fortunate in securing the enthusiastic assistance of my friend Mr.

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[1] I believe that it may cost about £50 to compute my table, but it is impossible as yet to give any precise estimate.

[2] The rigorous definition of the fictitious satellite is that its right ascension is equal to the moon's true longitude measured in her orbit.

Allnutt, and I owe him my warm thanks for the laborious computations he carried out. After computing fully half of the original tables, he made a comparison for the whole of 1889 between our predictions and those of the Indian Government.

The theory of the several methods of tidal prediction has hitherto taken up all our attention, and the reader will naturally be interested to know what degree of accuracy is attained by all this labour.

Captain Wharton has kindly supplied me with a comparison between the predictions, made by the synthetic method, for Portsmouth, and the curves registered by the tide gauge for the month of August 1886.

I find that the errors are as follows:—

Time		Height	
Magnitude of error. m. m.	No. of cases.	Magnitude of error in inches.	No. of cases.
0 to 5	25	0, 1, 2, 3	16
5 to 10	21	4, 5, 6	16
10 to 15	10	7, 8, 9	13
17	1	10, 11	8
32	1	13, 14	3
		16...	1
		18...	1
	58		58

The table of errors in the height, however, gives much too favourable an impression of the success of the prediction, for the prediction is so generally higher than the actuality, that the mean of all the predictions is 5½ inches higher than the truth.

I find that the probable error in the time is 4m.; that is to say, half the actualities fall within 4m. on each side of the prediction. Again, allowing 6 inches for the systematic error in the heights, I find that the probable error in the heights is 3½ inches. The average amplitude of oscillation of sea-level at spring tide (or the spring rise) is 13 feet 6 inches, and at neap tide (or the neap range) is 7 feet 9 inches. The errors in height are therefore not considerable compared with the total changes.

Let us next consider a case where the predictions were made by the tide-predicting machine. Colonel Baird sent me some time ago a comparison, made in the office of the Survey of India, between the Aden predictions and actualities for the year 1884.[1]

Taking the month of January as probably a fair sample, I find that the errors of the 57 high water predictions of that month were as follows:—

Time		Height	
Magnitude of error. m. m.	No. of Cases.	Magnitude of error. in.	No. of cases.
0 to 5	24	0	8
5 to 10	24	1	17
10 to 15	7	2	9
19	1	3	11
21	1	4	7
		5	5
	57		57

Mr. Allnutt determined the probable errors in time and height of the 698 high water and 698 low water predictions for the whole year, and found ± 4m. in time, and ± 2 inches in height, for both high and low waters.

On account of the magnitude of the diurnal tide at Aden, it is useless to state the spring rise of water, but about spring tide the range from high water to low water often amounts to 6 feet 6 inches or 7 feet. Hence, we may consider that an error of 1 inch at Aden has about the same relative importance as 2 inches at Portsmouth. Although the tidal constants of Aden were not so well determined in 1884 as they are now, I think the Aden predictions are better than those at Portsmouth—at least if the month of August 1886 is a fair sample of the latter port. It is fair, however, to state that the disturbance due to the irregular winds of our latitudes is very considerable at Portsmouth;[1] whereas the effects of the regular day and night breezes which occur at such a place as Aden are included in the tide-table.

It is a remarkable fact, at once creditable to the Indian Government and discreditable to all others, that the tides round the enormous range of coast of the Indian Ocean are better known than over any other area in the world.

Finally, let us consider our general tide-table, which has as yet only been worked out for high water at Aden. For the purpose of comparison, I computed a tide-table from March 10 to April 9, and from November 12 to December 12, 1884. In these periods there are 117 actual high waters, and one evanescent high water. Where a tide is evanescent—that is to say, when there is no rise of water—the general tide-table necessarily assigns a definite time for high water, although the rise of water may be *nil*. The following table classifies the errors of the (retrospective) predictions, without reference to signs:—

Time		Height	
Magnitude of error. m. m.	No. of cases.	Magnitude of error. in.	No. of cases.
0 to 5	35	0	22
5 to 10	34	1	42
10 to 15	23	2	26
15 to 20	10	3	17
20 to 25	7	4	3
25 to 30	3	5	5
39 to 46	4	6	1
108	1	7	1
Evanescent ...	1	Evanescent ...	1
	118		118

Omitting the cases of evanescence and of 108m. (which will be shown to be justifiable), the probable errors are 8m in time,[2] and 1·4 inches in height.

When the rise from the higher low water to the lower high water is *nil*, there is evanescence, and when that rise is small there is approximate evanescence.

I have accordingly examined the 24 cases in which the time-error is greater than 15m. (there being two cases of 15m. error exactly), and the following table gives the results:—

Rise from L.W. to H.W.	Time errors.
Nil	(Evanescence)
2 in.	108m.
6 in. to 11 in. ...	22, 22, 39, 44, 46m.
14 in. to 18 in. ...	16, 17, 22, 28, 29, 40m.
19 in. to 29 in. ...	16, 18, 18, 18, 19, 19, 20, 21, 21, 22, 22m.
3 ft. 6 in.	25m.

When we consider that the rise often amounts to 7 feet, all these tides, excepting the last, must be considered as approximating to evanescence. I suspect that the last of these errors must have been due to wind.

We have seen in Fig. 2 the sort of hang there is in the water in the case of an evanescent tide, and, accordingly, in approximate evanescence an error of half an hour, or even an hour, is quite insignificant.

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[1] I have recently learnt that the method of comparison in use at that time was such that there was a bias in favour of the prediction. It is not easy to read the time of high water on a tide-curve with accuracy, and previous knowledge of the predicted time gives the observer a bias in favour of a particular reading. This faulty method of testing the predictions has long since been superseded.

[1] I have no space to discuss another aspect of tidal prediction—namely, the influences of wind and weather on the tides; this is in itself a subject of considerable magnitude, and one which is not yet completely understood.

[2] There are circumstances connected with the alleged time-actuals for November 1884, which appeared to demand further inquiry, and I wrote to India accordingly. Colonel Hill, R.E., has now sent me a re-measurement of the tide-curves, but it is not possible to complete the examination of the results in time for this paper. I may state, however, that the comparison in the text leads to a substantially correct estimate.—*March* 20, 1891.

If these 24 cases be subtracted, the probable error in the time falls to 7m.

These retrospective calculations are distinctly better as regards height than those of the Indian Government, but are worse in the times. It is proper, however, to remark that better values of the tidal constants were used than were accessible in 1884.

The comparison between our predictions and the Indian ones made for the whole of 1889 by Mr. Allnutt, led to conclusions very similar to those just explained.

It may be concluded from these comparisons that highly satisfactory tide-tables may be made from our general tide-tables, when the tidal constants are accurately known.

If the general tide-table were computed in the full form, both for high water and for low water, it would cover 36 pages of rather small type. But at many places it may not be thought worth while to proceed to the full degree of accuracy. If the computation were made for every half-hour of moon's transit, and for every fortnight, the table would be reduced to 16 pages; and if high waters only were given, to 8 pages.

I believe, however, that this is not the direction in which abridgments may be best made, but that it would be more advantageous to drop some of the corrections. Thus the correction to the height, which depends on the longitude of the moon's node, and a certain small correction to the time, which depends on a portion of the equation of time (of which no mention has been made), might be dropped without much loss of accuracy. If the tables are still supposed to be half-hourly and fortnightly, these omissions might reduce them to 6 pages. At some ports a rough high water tide-table might even be contained in 2 or 3 pages, but in this case it might be necessary to add a warning of the possibility of sensible error in the times and heights of the lower high water.

The question of how far to go in each case will depend on a variety of circumstances. The most important consideration is, I fear, likely to be the amount of money which can be spent on computation and printing; after this will come the degree of desirability of a trustworthy tide-table, and the accuracy with which the tidal constants are known—for it is obviously useless to form a nominally accurate tide-table from inaccurate values.

My aim has been to reduce the tables to a simple form, and if, as I imagine, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, the principal object in view has been attained. G. H. DARWIN.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

SUFFICIENT time has elapsed since the dismissal of the International Congress to permit a deliberate view to be taken of the results of the meeting, and to determine how far the prospects of the photographic chart have been improved by the resolutions which the Committee has entered on its minutes. But there is the preliminary consideration, that it is now four years since the scheme was definitely conceived and resolved upon. Have those four years been profitably employed? Doubtless the initiatory steps, such as procuring the necessary instruments, and acquiring facility in their use, as well as a knowledge of their capacities, have occupied a longer time than was originally anticipated, but seen in the light of a fuller experience the completion of so many details in a shorter time was not warranted. It is a matter of legitimate congratulation that all the participating Observatories are adequately and similarly equipped, and that a work undertaken in hope, rather than founded on experience, is moving surely and certainly to a successful issue, and strictly on the lines originally contemplated. The early resolutions of the Committee were not guided by trial and practice, and it is fortunate that so little has had to be altered, and that the scheme in its integrity remains practically as it was conceived. The words with which Admiral Mouchez closed the Conference on the fourth day of debate happily described the situation, and the attitude of the various members of the Committee. "You have had to treat difficult questions," said he; "you have treated them without reference to any other thought than that of truth, and the progress of science. Divergences of views have exhibited themselves as to the means to be employed, never as to the end to be attained. These divergences were inevitable; they were even necessary; they have only made us see the questions in a clearer light, and have never disturbed the cordiality of your relations. The unanimity which has marked your final decisions is a certain pledge of ultimate success. It is in this conviction that I declare the Conference of 1891 closed, and the work of the photographic chart of the heavens commenced."

The keynote of one of these "divergences of views" was struck by the Admiral himself in his opening address, and the subsequent debate led to the exhibition of considerable excitement on the part of individual members of the Committee, which has been commented on elsewhere. It is well known that under the auspices of the International Committee it is proposed to effect two objects, each very desirable in itself. One of these, the one which really called the Committee into existence, is the photographic representation of the present condition of the heavens, in which shall be delineated stars which, under a certain definition, are called the fourteenth magnitude. The other, a more recent thought, is the preparation of a catalogue in which shall be exhibited the co-ordinates of stars down to the eleventh magnitude, on the scale of Argelander's zones. The question arose, as Admiral Mouchez pointed out, whether these two distinct and separate objects should be proceeded with *pari passu*, or whether, since a shorter time would be sufficient to furnish the plates from which the catalogue would be deduced, it was desirable that the data for the catalogue should be supplied before the more arduous undertaking of the chart plates should be commenced. Admiral Mouchez, in his opening address, clearly indicated the advisability of the latter method of proceeding, and adduced very excellent arguments for such a course. A catalogue, as he remarked, is immediately useful, and is a want among astronomers of the present day, whereas the chart is really intended for the benefit of future astronomers, who will value it the more in proportion to its completeness. Further, there are reasons to believe that the duration of exposure necessary to secure, on the negative, a black measurable image of a star of the fourteenth magnitude, will be much longer than has hitherto been anticipated, and that consequently a much greater expenditure of time will be necessary for the production of the plates than has generally been imagined. If, then, the two series of plates are obtained simultaneously, the shorter exposure plates will be injudiciously delayed, and the manufacture of the catalogue cannot be proceeded with till the chart plates of long exposure are completed—a course manifestly to the detriment of the present condition of astronomical science. On the other hand, if by co-operation the series of negatives of short exposure be secured in about two years, which is quite possible, the hands of astronomers will be greatly strengthened by the welcome which will be accorded to the catalogue, and they will be encouraged to undertake the more onerous part of the work. Additional experiments and information could be collected, and it is not too much to hope that some improvement in the preparation of photographic films might secure